

Structural Change in Zambian Bank Capital, Portfolio Risk and Fragility: Evidence around the 2012 Capital Reform and the 2018–2021 Macro-Financial Episode

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Abstract

This paper investigates how capital, portfolio risk and banking-sector fragility in Zambia changed around two important episodes: the 2012 minimum-capital reform and the macro-financial stress that intensified from 2018. The reform raised minimum primary paid-up capital from K12 million to K104 million for locally owned banks and K520 million for foreign-owned banks, with the final compliance deadline extended to December 2013. The analysis draws on 180 monthly observations covering 33 published banking-sector indicators between January 2011 and December 2025. It constructs a sectoral fragility index using principal component and entropy weights and recovers risk-weighted asset density from published capital ratios. Structural change is assessed through sup-Wald tests with autoregressive bootstrap inference, multiple-break detection, fixed-date segmented regressions, information-criterion comparisons, quarterly aggregation, event-time profiles and placebo date checks. The evidence points to statistically detectable and economically plausible adjustment during the 2013 compliance period: total regulatory capital relative to risk-weighted assets increased, while several measures of portfolio density, credit concentration and leverage declined. This pattern is consistent with reform-related balance-sheet adjustment. It cannot, however, be attributed solely to the reform because several series were already changing before the deadline and placebo dates often yield comparable statistics. When the break date is allowed to emerge from the data, the clearest changes in most capital and portfolio-risk series cluster between September and November 2018. That timing accords with the emergence of the sovereign-bank nexus and a broader deterioration in macro-financial conditions, although aggregate data cannot separate sovereign stress from exchange-rate, fiscal, monetary and credit-cycle forces. The study therefore offers evidence of comparative structural change rather than causal treatment effects. Its central finding is straightforward: the 2012–2013 reform is visible in Zambia's aggregate banking indicators, but the later macro-financial episode produced the broader and more prominent set of breaks.

1. Introduction

Whether stronger capital requirements actually make banks safer remains one of the most important and least straightforward questions in prudential regulation. Theory gives no single answer. In the portfolio models developed by Koehn and Santomero (1980) and Kim and Santomero (1988), a binding capital constraint may encourage a utility-maximising bank to move towards assets offering higher returns at greater risk. In that setting, the probability of failure can rise even when the measured capital ratio improves. Furlong and Keeley (1989), working within an option-value framework, reach the opposite conclusion: additional capital reduces the value of the deposit-insurance put and weakens the incentive to gamble. Rochet (1992) helps reconcile these positions by showing that the result depends on the bank's objective and on whether the requirement is absolute or appropriately risk-weighted. The effect of capital regulation is therefore an empirical question, shaped by policy design and by the adjustment options available to affected banks.

Zambia's 2012 reform offers a useful setting in which to examine the timing of aggregate balance-sheet adjustment. On 24 January 2012, the Minister of Finance announced that minimum primary paid-up capital would rise from K12 million to K104 million for locally owned banks and K520 million for foreign-owned banks. The measure took legal effect on 30 January 2012. Although compliance was initially required by December 2012, the deadline was later extended to December 2013. Two features are especially important: the requirement was stated as an absolute kwacha amount rather than a risk-weighted ratio, and it differed by ownership. These features suggest several plausible adjustment dates, but they do not make the policy an unanticipated or exogenous shock. For that reason, this study treats the announcement and compliance dates as institutionally meaningful reference points, not as randomly assigned treatment dates.

The analysis addresses two related questions. First, when do Zambia's published banking-sector indicators show structural change in relation to the reform announcement, the original deadline and the extended compliance deadline? Second, how do the timing and direction of those changes compare with the breaks observed during the macro-financial deterioration that began in 2018? This comparison provides economic context. It does not treat a Wald statistic as an effect-size measure, nor does it assume that every later break arose solely from sovereign stress.

The empirical approach is designed to locate and describe structural change before asking what interpretation the data can reasonably support. The Andrews (1993) sup-Wald procedure searches for an unknown break, with critical values obtained from an autoregressive bootstrap under the null of no break. Bai Perron analysis then allows for more than one regime. Fixed-date Wald statistics and Bayesian information criteria compare the three policy dates, while segmented regressions describe changes in levels and trends at the extended deadline. Quarterly estimates, event-time profiles, placebo-date comparisons and comparison outcomes provide additional checks. Because the evidence comes from a single aggregate banking system without an untreated counterfactual, the estimates are interpreted as temporal associations rather than causal effects.

The paper makes four contributions. First, it provides a monthly account of change in Zambia's published banking-sector indicators over 2011–2025. Second, it separates the announcement, original deadline and extended deadline instead of forcing the reform into a single date. Third, it shows how risk-weighted asset density can be recovered from two published capital ratios, provided their accounting definitions are consistent. Fourth, it evaluates the reform-period changes alongside the later macro-financial episode using the same variables and specifications. The contribution is deliberately comparative and descriptive: the analysis identifies when and how the aggregate indicators changed, but it does not claim to recover either the reform's treatment effect or an isolated causal effect of sovereign distress.

The remainder of the paper is organised as follows. Section 2 describes the institutional setting. Section 2 reviews the relevant theory and econometric literature. Section 2 describes the data and the construction of the derived variables. Section 3 sets out the econometric strategy. Section 4 reports results. Section 5 concludes

2 Literature Review

2.1 The 2012 Minimum Capital Reform

2.

Before 2012, every Zambian commercial bank faced the same minimum primary-capital requirement of K12 million, equivalent at the time to about US\$3.5 million. The reform replaced that uniform threshold with a two-tier system: K104 million (approximately US\$20 million) for locally owned banks and K520 million (approximately US\$100 million) for foreign-owned banks. In practical terms, the new requirement was almost nine times the previous level for local banks and more than forty-three times the previous level for foreign-owned institutions.

Table 1: Timeline of the reform and of the comparison episode

Date	Event	Relevance
24 January 2012	Announcement in the budget address	Information arrives
30 January 2012	Revision takes legal effect	Legal commencement
31 December 2012	Original compliance deadline	First binding date
31 December 2013	Extended compliance deadline	Effective binding date
2013 to 2014	Six foreign owned banks convert to local status	Margin of adjustment
2018 onwards	Onset of sovereign debt distress	Comparison shock

Source: compiled by the authors from Bank of Zambia sources and contemporaneous reporting.

Two features of implementation matter for the empirical design. The first is the extension of the compliance deadline from December 2012 to December 2013. Banks could begin adjusting after the announcement and continue throughout the transition. The final deadline should therefore not be read as a sudden, wholly unexpected shock.

The analysis responds to this institutional reality by comparing several policy dates and examining whether movement was already under way before the extended deadline.

The second feature is the ownership-based difference in capital thresholds, which meant that banks did not face the same degree of pressure. Contemporary reports suggest that some foreign-owned institutions altered their ownership classification rather than meet the substantially higher foreign-bank requirement. This was an important adjustment channel. Sector-level data, however, cannot show which institutions were genuinely constrained, so any interpretation of bank behaviour must remain aggregate and depend on the accuracy of the institutional record.

The comparison episode begins in 2018, as Zambia's fiscal and external vulnerabilities became more pronounced and eventually culminated in the 2020 Eurobond default. Banks' exposure to government securities offers a plausible channel through which sovereign stress could affect the financial system. Yet this period also saw exchange-rate depreciation, shifts in interest rates and government-security yields, slower growth, the COVID-19 shock and regulatory responses. It is therefore described here as the 2018–2021 macro-financial episode. Breaks observed during these years are consistent with a sovereign-bank nexus, but they should not be interpreted as a sovereign-stress effect cleansed of every other shock.

2.2 Theory and Econometric Framework

Capital requirements and portfolio risk

The portfolio approach of Koehn and Santomero (1980) treats the bank as a mean-variance optimiser choosing assets subject to a leverage constraint. Tightening the constraint moves the bank along its efficient frontier towards a higher-risk composition, and for banks with sufficiently low risk aversion the increase in asset risk outweighs the reduction in leverage, raising the probability of failure. Kim and Santomero (1988) demonstrate that the perverse outcome is avoidable if the constraint is expressed in risk-weighted terms with correctly chosen weights. The Zambian requirement is an absolute kwacha floor and therefore corresponds precisely to the case in which the perverse response is theoretically available, which makes the sign of the estimated risk

response informative rather than foregone.

Furlong and Keeley (1989) obtain the opposite result by treating bank equity as a call option on assets. Higher capital reduces the value of the deposit insurance put, so a better capitalised bank has less to gain from raising asset risk and the incentive to gamble falls. Rochet (1992) reconciles the two frameworks by showing that the sign depends jointly on the bank's objective and on the structure of the constraint.

Empirical work following Shrieves and Dahl (1992) emphasises that capital and risk are jointly determined. This insight is important for interpretation, but contemporaneous changes in aggregate capital and recovered risk density cannot be given structural coefficients without valid excluded instruments. This paper therefore uses theory to interpret the direction of observed changes and does not claim to identify a simultaneous behavioural system.

Detecting and dating structural change

Testing for a break when its date is unknown raises the well-known problem that the break parameter is unidentified under the null, so that conventional Wald statistics do not follow a chi-square distribution. Andrews (1993) derives the limiting distribution of the supremum of the Wald sequence over a trimmed set of candidate dates, and Andrews and Ploberger (1994) develop the corresponding optimal average and exponential statistics. Bai (1997) provides the asymptotic distribution of the estimated break date and thereby the basis for confidence intervals, and Bai and Perron (1998, 2003) extend the framework to multiple breaks with efficient computation by dynamic programming.

Monthly prudential ratios are highly persistent, which can make standard asymptotic critical values unreliable in a sample of this size. The analysis therefore relies on an autoregressive bootstrap. One limitation is especially important. With 15 per cent trimming, roughly the first and last 27 months of the 180-month sample are excluded from the unknown-break search. Since the data begin in January 2011, the procedure cannot select the January 2012 announcement as a break date. It remains useful for identifying later changes, but silence on January 2012 is not evidence that nothing happened then.

Known-date segmented regressions provide a necessary complement to the unknown-break procedure. The placebo-date exercise asks a simple question: how often would another admissible date in the same series generate a statistic at least as large as the one observed at the policy date? Because the policy date was not randomly assigned, this is not Fisher-exact randomisation inference. It is better understood as a diagnostic of whether the chosen date is unusual within a persistent and evolving series.

Evidence from African banking systems

Evidence on capital regulation in African banking systems remains comparatively thin and is dominated by country reforms that combined recapitalisation with consolidation, ownership change or strengthened supervision. These experiences show why aggregate before-and-after comparisons require caution: capital regulation can affect entry, exit, ownership, lending composition and reported ratios simultaneously. Bank of Zambia financial-stability reporting identifies strong capital buffers alongside vulnerabilities related to credit concentration, dollarisation and the sovereign-bank nexus. This paper contributes a high-frequency Zambian description while recognising that published sector aggregates cannot recover heterogeneous bank-level treatment effects.

3 Data and Methodology

3.1 Data and Variable Construction

Source, coverage and treatment

The data are the Bank of Zambia banking sector key performance indicators, comprising thirty three financial soundness indicators reported monthly from January 2011 to December 2025, giving one hundred and eighty observations. The indicators span five CAMELS dimensions: five capital adequacy measures, twelve asset quality measures, eight earnings measures, five liquidity measures and three measures of sensitivity to market risk. Management quality is not directly quantifiable in published returns and the efficiency ratio serves as its conventional proxy.

Three data problems were addressed before estimation. First, the capital-related ratios show a common scaling anomaly in March 2012: ratios with capital in the numerator collapse, while those with capital in the denominator increase by roughly an order of magnitude. These observations were treated as missing and linearly interpolated. The same approach was used for isolated implausible values in loan and deposit concentration. The annualised net-interest-margin series was excluded because of reporting discontinuities. The March 2012 correction deserves particular caution because it falls close to the reform announcement. Announcement-period findings should therefore be checked against deletion, alternative interpolation and state-space smoothing once the underlying data and code are archived.

Recovery of risk-weighted asset density

The standard measure of portfolio risk in the capital and risk literature is the ratio of risk-weighted to total assets. It is not published directly but is recoverable exactly from the two published capital ratios, since the tier one capital term cancels:

$$RWA / TA = (Tier\ 1\ capital / TA) \div (Tier\ 1\ capital / RWA) \quad (1)$$

This identity is valid only when both published ratios use the same Tier 1 capital numerator and compatible asset definitions. The recovered series averages 45.75 per cent, with a standard deviation of 3.18 percentage points and a range from 39.91 to 53.12 per cent. These values are economically plausible, although the derivation should ultimately be verified against the regulatory definitions and, where available, independently reported totals for risk-weighted assets.

The sectoral fragility index

Seventeen indicators spanning all five available CAMELS dimensions were signed so that higher values denote greater fragility, winsorised at the first and ninety-ninth percentiles to limit outlier influence, and standardised. Two weighting schemes were applied, following established composite indicator practice (Nardo et al., 2005) and its supervisory application (Galan, 2021). Principal component weighting retains components with eigenvalues exceeding unity and weights them by their share of retained variance; four components satisfy the criterion and together account for 82.0 per cent of total variance. Entropy weighting assigns each indicator a weight proportional to its information divergence,

$$wk = (1 - ek) / \sum_l (1 - el), \quad ek = - (1 / \ln N) \sum_t p_{kt} \ln p_{kt} \quad (2)$$

The correlation between the two indices is 0.611. Rather than showing that the measures are interchangeable, this result indicates that the choice of weights materially shapes the latent fragility measure. Both versions are therefore retained, and a finding is described as robust only when they point in the same direction. A replication appendix should list the 17 indicators and their signs, transformations, PCA loadings, entropy normalisation and leave-one-indicator-out sensitivity results.

3.2 Econometric Strategy

Breaks at unknown timing

For each outcome the maintained regression under the null of no break is a deterministic trend with controls for sector conditions. Against a break at candidate date b the alternative admits a shift in both level and trend,

$$Y_t = \alpha + \beta_1 t + \beta_2 D_t(b) + \beta_3 (t - b) D_t(b) + \theta' X_t + \varepsilon_t \quad (3)$$

where D takes the value one from date b onwards and X contains cumulative return on assets and the liquid asset ratio. The Wald statistic for the joint null that β_2 and β_3 are zero is computed at every candidate date within fifteen per cent trimming of each sample end, and the test statistic is its supremum,

$$\sup W = \max_{b \in [0.15n, 0.85n]} W(b) \quad (4)$$

Critical values are obtained by bootstrap. Residuals from the no-break model are represented by an autoregression of order three, and 399 artificial series are generated under the no-break null. The sup-Wald search is repeated for each artificial series and the bootstrap probability is the proportion of statistics at least as large as the observed value. Because 399 replications provide limited tail resolution, the reported probabilities should be regarded as provisional and increased to at least 999, preferably 4,999, in the final reproducible estimation. Multiple breaks are located by minimising residual sums of squares over admissible partitions, with the number of regimes selected by the Bayesian information criterion. The maximum number of breaks, minimum segment length and criterion values for alternative break counts should be supplied in a replication appendix.

Adjudicating among policy dates

Because the reform has three defensible dates, the same level-and-slope specification is evaluated at each date. Wald statistics are used to assess statistical evidence against parameter stability, and Bayesian information criteria compare fit among models with identical parameter counts. Neither measure is interpreted as an economic effect size. Comparisons involving January 2012 are especially tentative because only about 12 pre-announcement observations are available, whereas dates near the sample centre are estimated with substantially more information on both sides.

Direction, magnitude and dynamics

At the preferred policy date, equation (3) is estimated with Newey–West heteroskedasticity- and autocorrelation-consistent standard errors using twelve lags. The level coefficient describes the estimated discontinuity and the interaction coefficient describes the change in trend. Quarterly aggregation assesses sensitivity to monthly reporting noise. A binned event-time profile, measured relative to the extended deadline and grouped in quarters over a 24-month window, describes adjustment before and after the deadline. Significant pre-period coefficients are treated as evidence against an instantaneous causal interpretation, even when anticipatory adjustment is institutionally plausible.

Placebo-date and comparison-outcome checks

Two checks discipline interpretation. First, placebo dates are drawn from the admissible interior of the sample and equation (3) is re-estimated at each date. The resulting empirical distribution indicates whether December 2013 is unusual relative to other candidate dates; it does not create experimental randomisation. Second, the same segmented specification is applied to earnings, funding and efficiency outcomes. These variables are labelled comparison outcomes, not outcomes wholly outside the reform's causal pathway, because capital regulation may indirectly affect each of them. Joint tests of pre-period event-time coefficients provide an additional check on whether adjustment began before the deadline.

4 Results

4.1 The sectoral fragility index

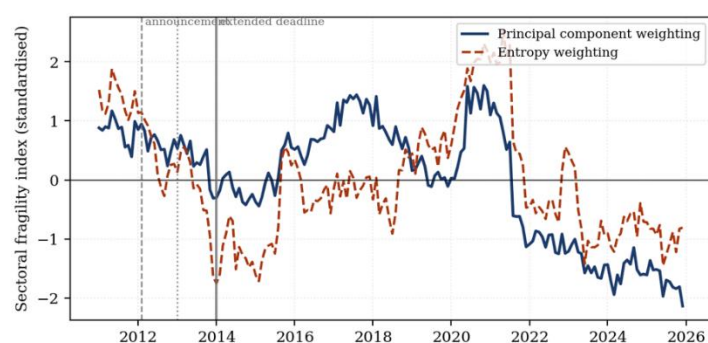


Figure 1: Sectoral fragility index under alternative weighting schemes, 2011 to 2025
Source: authors' computation.

The two indices agree on the broad profile. Fragility was elevated at the start of the sample, declined through 2013 and 2014, rose to a local peak in 2017 under principal component weighting, deteriorated sharply in 2020, and has fallen steadily since 2022 to the lowest levels recorded. The principal component index averages 0.831 in 2011 and minus 1.713 in 2025. The two diverge in the middle of the sample, with the entropy index identifying 2020 and 2021 as the most fragile years against 2017 for the principal component variant, a divergence that reflects the different weight each scheme places on asset quality relative to capital.

4.2 Structural change at unknown timing

Table 2 reports the sup-Wald test with the break date left free.

Table 2: Sup-Wald test for a break at unknown date, fifteen per cent trimming

Outcome	supWald	Break date	95% interval	Bootstrap p
Large loan exposure to capital	423.21	Nov 2018	Nov 2018	0.005
RWA density	353.05	Sep 2018	Sep 2018	0.005
Total capital to RWA	288.50	Sep 2018	Sep 2018	0.015
Tier 1 capital to RWA	283.36	Sep 2018	Sep 2018	0.020
Insider loans to capital	237.11	Oct 2018	Sep to Oct 2018	0.010
Fragility index (entropy)	234.15	Oct 2018	Sep to Nov 2018	0.020
Twenty largest loans to loans	215.32	Oct 2019	Oct 2019	0.010
Loan to deposit ratio	156.28	Aug 2021	Aug to Sep 2021	0.188
Gross non-performing loan ratio	143.84	Jan 2017	Dec 2016 to Feb 2017	0.087
Fragility index (PCA)	59.06	Apr 2013	Apr to Jun 2013	0.310

Source: authors' computation.

For most capital and portfolio-risk measures, the unrestricted search places the strongest break between September and November 2018. Bootstrap probability values for these series range from 0.005 to 0.020. The twenty-largest-loans measure instead breaks in October 2019, and the loan-to-deposit ratio in August 2021. The PCA fragility index selects April 2013, although its bootstrap probability is 0.310. Taken together, the results show that the dominant breaks cluster in the later macro-financial episode. They do not show that sovereign stress was their only cause. It is also worth repeating that the trimming rule excludes January 2012, so Table 2 cannot settle the question of an announcement-date break.

Figure 2 displays the underlying Wald sequences for two representative series.

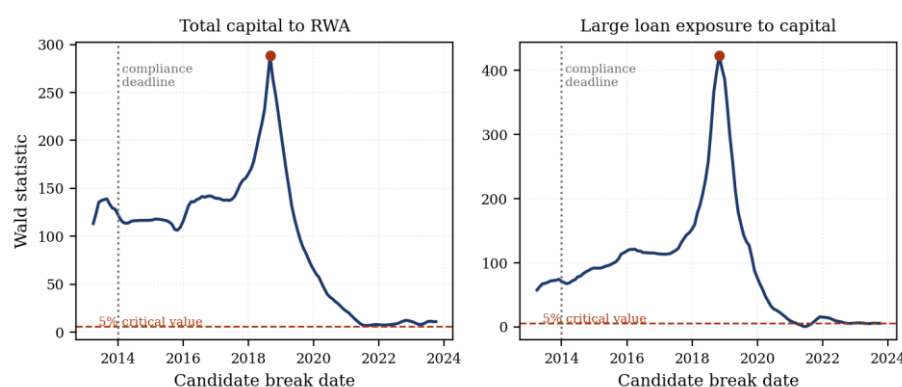


Figure 2: Wald statistic across candidate break dates

Source: authors' computation.

The Wald sequences remain high across broad portions of the admissible range, a pattern that reflects both persistence and the difficulty of assigning change to one precise month. The peaks occur in 2018, although large-loan exposure also rises noticeably around the compliance period. These statistics measure evidence against parameter stability; they do not measure how far a bank's balance sheet moved in economic terms.

Table 3 reports the multiple break analysis, which allows for several regimes rather than one.

Table 3: Multiple structural breaks, three breaks selected by information criterion

Series	Break 1	Break 2	Break 3
Risk-weighted asset density	May 2013	Sep 2018	Apr 2021
Large loan exposure to capital	Dec 2013	Nov 2018	Dec 2021
Loan to deposit ratio	Jun 2013	Dec 2016	Aug 2020
Tier 1 capital to risk-weighted assets	Aug 2015	Sep 2018	Aug 2021
Gross non-performing loan ratio	Mar 2015	Aug 2018	Aug 2021
Fragility index (principal component)	Sep 2015	Jan 2019	Aug 2021

Source: authors' computation.

Allowing multiple regimes identifies changes during the reform-compliance period in risk-weighted asset density, large-loan exposure and the loan-to-deposit ratio, followed by additional breaks during 2018–2021. This pattern is consistent with phased reform adjustment followed by a broader macro-financial regime change. The break dates should still be reported with confidence intervals and accompanied by the criterion values used to select three breaks. No inference about January 2012 can be drawn from the trimmed unknown-break search.

4.3 Which policy date does the data prefer?

Table 4 evaluates the Wald statistic at each candidate policy date and at the 2018 break.

Table 4: Wald statistics at fixed dates

Outcome	Jan 2012	Dec 2012	Dec 2013	Sep 2018	Best
Fragility index (PCA)	17.41	36.10	15.82	20.27	Dec 2012
Fragility index (entropy)	31.67	86.48	172.47	230.71	Dec 2013
RWA density	13.82	21.77	19.47	353.05	Dec 2012
Total capital to RWA	36.81	78.34	128.34	288.50	Dec 2013
Tier 1 capital to RWA	34.84	87.80	121.43	283.36	Dec 2013
Large loan exposure to capital	21.60	36.72	74.38	366.02	Dec 2013
Twenty largest loans to loans	15.78	15.24	35.32	104.91	Dec 2013
Insider loans to capital	65.98	96.62	138.14	233.83	Dec 2013
Loan to deposit ratio	88.55	125.32	121.64	128.15	Dec 2012
Gross non-performing loan ratio	11.23	34.65	40.63	9.92	Dec 2013
Mean	33.79	61.90	86.76	201.87	

Source: authors' computation.

Of the three policy dates imposed on the model, December 2013 produces the largest Wald statistic for seven of the ten outcomes; December 2012 is preferred for the remaining three. January 2012 is never preferred. This ranking is consistent with adjustment becoming more pronounced as compliance approached. It is not, however, a clean contest between announcement and deadline effects: only about one year of pre-announcement data is available, and banks may have adjusted gradually over the entire transition.

For most outcomes, the fixed-date Wald statistic is larger in September 2018 than at any policy date. The average is 201.87 in September 2018, compared with 86.76 in December 2013. This is stronger statistical evidence of instability around 2018, not evidence that the later episode had 2.33 times the economic effect. Wald statistics also reflect residual variance, persistence, covariance and the location of the candidate date within the sample. The non-performing-loan ratio is the notable exception, with a larger statistic at the compliance deadline than in September 2018.

Table 5 confirms the ranking among policy dates by information criterion.

Table 5: Bayesian information criterion by candidate policy date

Outcome	Announcement Jan 2012	Original deadline Dec 2012	Extended deadline Dec 2013
Fragility index (PCA)	421.7	418.1	409.7
RWA density	878.7	875.1	865.5
Tier 1 capital to RWA	831.9	812.9	805.4
Total capital to RWA	819.4	797.5	782.8
Large loan exposure to capital	1834.0	1831.2	1828.8
Twenty largest loans to loans	1052.3	1045.6	1028.6
Insider loans to capital	1016.1	982.1	972.7
Loan to deposit ratio	1062.7	1044.8	1037.4
Gross non-performing loan ratio	841.1	841.4	833.9

Source: authors' computation.

When attention is restricted to the three policy dates, the model using the extended deadline has the lowest Bayesian information criterion for every outcome. December 2013 is therefore the best-fitting policy reference date within this set of segmented regressions. That finding does not turn the deadline into an exogenous discontinuity, especially because the event-time evidence shows substantial movement before it.

4.4 Direction and magnitude of the adjustment

Table 6: Segmented regression at the extended compliance deadline

Outcome	Level shift	p	Slope change	p	adj R ²
Capital adequacy					
Total capital to RWA	2.957	0.013	-0.148	0.004	0.466
Tier 1 capital to RWA	2.308	0.083	-0.173	0.002	0.502
Risk-taking					
Risk-weighted asset density	-3.085	0.002	-0.131	0.003	0.660
Large loan exposure to capital	-50.188	0.008	0.839	0.171	0.549
Twenty largest loans to loans	-5.104	0.004	0.061	0.355	0.850
Insider loans to capital	-2.637	0.001	0.286	0.000	0.682
Loan to deposit ratio	-4.960	0.004	-0.464	0.000	0.925
Fragility and asset quality					
Fragility index (PCA)	-0.514	0.065	-0.016	0.220	0.901
Gross non-performing loan ratio	-0.801	0.260	0.048	0.043	0.885

Source: authors' computation.

The direction of the estimated changes at the extended deadline is broadly consistent with the option-value view. Total regulatory capital relative to risk-weighted assets increases by 2.96 percentage points. At the same time, risk-weighted asset density, large-loan exposure, the share of the twenty largest

loans, insider lending and the loan-to-deposit ratio all decline. The PCA fragility index falls by 0.51 standard deviations at the 10 per cent level, whereas the non-performing-loan ratio shows no significant shift. These are conditional segmented-regression estimates, not treatment effects. Their shared direction makes a reform-related interpretation plausible, but contemporaneous developments and earlier trends rule out exclusive attribution.

Negative post-deadline slope changes on the capital ratios indicate that the initial increase was not maintained at the same rate. This may reflect balance-sheet growth after compliance, but that explanation cannot be distinguished from other changes in assets, risk weights or sector composition using aggregate data.

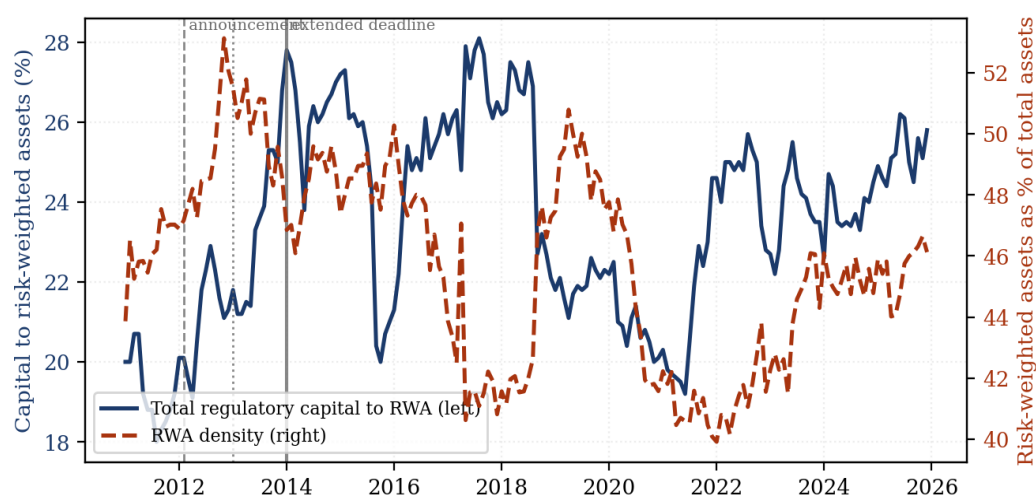


Figure 3: Regulatory capital and risk-weighted asset density, 2011 to 2025

Source: authors' computation.

The plotted series help place these estimates in context. Capital rises through 2012–2014, declines through 2019 and recovers after 2021. Risk-weighted asset density follows a related early pattern and changes markedly from 2018. Visual inspection supports the presence of distinct regimes, but it also shows gradual movement rather than a single clean discontinuity at the compliance deadline.

Table 7: Quarterly replication of the level shift

Outcome	Level shift (quarterly)	p
Risk-weighted asset density	-3.202	0.004
Total capital to RWA	2.733	0.030
Large loan exposure to capital	-44.574	0.021
Twenty largest loans to loans	-4.445	0.012
Insider loans to capital	-2.833	0.002
Fragility index (PCA)	-0.604	0.041

Source: authors' computation.

Quarterly point estimates are close to the monthly estimates and retain similar signs. This reduces concern that the findings are driven entirely by monthly reporting noise, although aggregation does not resolve confounding or anticipation.

4.5 Dynamics and placebo date evidence

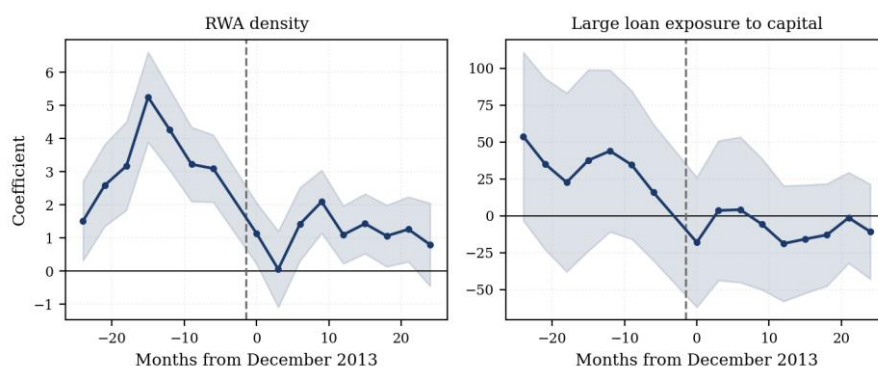


Figure 4: Event-study coefficients around December 2013, quarterly bins

Source: authors' computation.

The event-time profiles reveal adjustment both before and after the extended deadline. Crucially, the joint null that all pre-period coefficients are zero is rejected for every outcome reported. This is more than a technical footnote: December 2013 cannot be treated as an isolated and unexpected

discontinuity. The earlier movement is compatible with phased adjustment following the 2012 announcement, but it also means that the deadline coefficients combine anticipation, continuing trends and any shocks occurring at the same time.

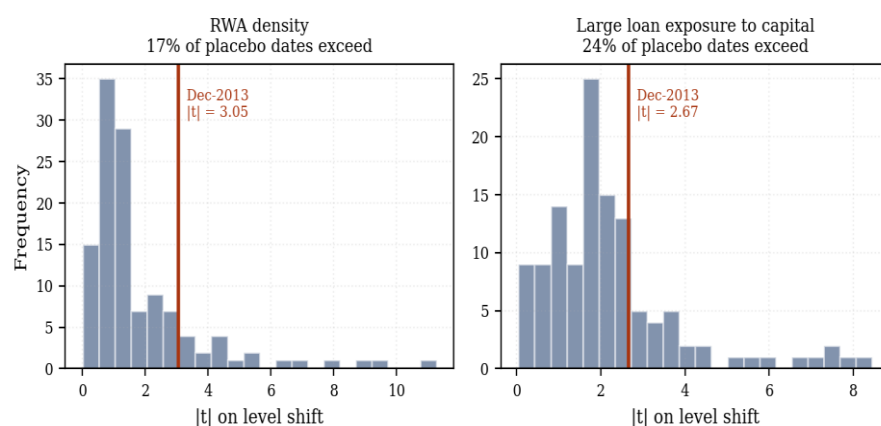


Figure 5: Distribution of test statistics across placebo break dates

Source: authors' computation.

Table 8: Placebo date comparison

Outcome	t at December 2013	Placebo dates reaching it
Insider loans to capital	3.18	27 of 100
Risk-weighted asset density	3.05	22 of 100
Loan to deposit ratio	2.89	21 of 100
Twenty largest loans to loans	2.87	29 of 100
Large loan exposure to capital	2.67	19 of 100
Total capital to RWA	2.48	26 of 100
Fragility index (PCA)	1.85	26 of 100
Tier 1 capital to RWA	1.73	31 of 100
Gross non-performing loan ratio	1.13	63 of 100

Source: authors' computation.

For most outcomes, between 19 and 31 per cent of sampled placebo dates produce statistics at least as large as those recorded in December 2013; for non-performing loans the share rises to 63 per cent. The extended deadline is therefore not exceptional against the background variation in these persistent series. This exercise is properly described as a placebo-date comparison, not exact randomisation inference. A final replication should also use every admissible date rather than a sample of 100.

4.6 Comparison-outcome evidence

Table 9: Segmented regressions for focal and comparison outcomes

Outcome	Level shift	p	Group
Risk-weighted asset density	-3.085	0.002	Focal
Total capital to RWA	2.957	0.013	Focal
Large loan exposure to capital	-50.188	0.008	Focal
Twenty largest loans to loans	-5.104	0.004	Focal
Insider loans to capital	-2.637	0.001	Focal
Earning assets to total assets	-4.142	0.000	Comparison
Return on equity (cumulative)	-2.452	0.003	Comparison
Net interest margin (cumulative)	-0.175	0.292	Comparison
Deposit concentration	0.959	0.461	Comparison
Efficiency ratio	1.013	0.579	Comparison
Core deposits to total deposits	0.266	0.778	Comparison

Source: authors' computation.

All five focal measures of capital and portfolio structure shift at the 5 per cent level, compared with two of the six comparison outcomes. The contrast lends some support to the interpretation, but it is not a decisive falsification test. Earning assets, return on equity, margins, funding and efficiency may themselves respond to capital constraints or to common macroeconomic conditions. The large statistic for earning assets is a reminder that the December 2013 period involved wider balance-sheet change; not every movement observed at that time can safely be assigned to the capital reform.

4.7 Synthesis

The evidence supports a measured conclusion. Several indicators of capital, concentration and leverage moved in economically coherent directions during the 2013 compliance period, and December 2013 fits the data better than the other two policy dates considered. This is consistent with reform-

related balance-sheet adjustment, but it is not a causal estimate. Banks were already adjusting before the deadline, many placebo dates generate similarly large statistics, and at least one comparison outcome changes sharply.

The unknown- and multiple-break results also point to a broader regime change over 2018–2021. Most capital and portfolio-risk series place their dominant break between September and November 2018, with further changes around 2020 and 2021. This timing is consistent with the sovereign-bank nexus described in financial-stability reports, but the same period also contains exchange-rate, fiscal, monetary, pandemic and credit-cycle forces that cannot be separated in the present design.

The most defensible comparison is therefore about how widespread and statistically prominent the changes were, not about ratios of causal effects. Zambia's 2012–2013 capital reform left a visible imprint on aggregate banking indicators. The later macro-financial episode, however, produced the dominant unrestricted breaks across a wider range of capital and portfolio-risk measures.

5 Conclusion

This study examined how Zambia's aggregate banking-sector indicators changed around the 2012 minimum-capital reform and the 2018–2021 macro-financial episode. Using 180 monthly observations, it combined two measures of sectoral fragility with a recovered measure of risk-weighted asset density, unknown- and multiple-break procedures, fixed-date segmented regressions, quarterly aggregation, event-time profiles and placebo-date comparisons.

Three conclusions stand out. First, December 2013 is the best-fitting policy reference date in the fixed-date models, and the movements in capital, portfolio density, concentration and leverage are consistent with adjustment to the reform. Second, neither the event-time evidence nor the placebo exercise supports a clean causal reading: movement began before the deadline, and several other dates yield comparable statistics. Third, when the break date is not imposed, most capital and portfolio-risk series select the macro-financial deterioration of 2018, with further regime changes around 2020–2021. The later episode is therefore more pervasive in the structural-break record. The aggregate design cannot, however, attribute it entirely to sovereign stress or express its magnitude as a multiple of the reform's effect.

The policy implications should be read with the same care. A transition period matters because banks can adjust throughout the interval between announcement and compliance; supervisors should track institution-level capital shortfalls and portfolio changes during implementation, not only at the final deadline. Absolute capital floors and risk-based ratios may operate through different balance-sheet channels, although this study cannot identify those effects separately. The concentration of later breaks also reinforces the case for monitoring sovereign exposure, credit concentration, foreign-exchange risk and liquidity together. In this respect, current Bank of Zambia reporting is right to place the sovereign-bank nexus within a broader macro-financial risk environment.

Five limitations define the boundaries of the findings. First, sector aggregates provide no untreated counterfactual and therefore cannot identify a causal treatment effect. Second, the sample begins only 12 months before the announcement, and the 15 per cent trimming rule excludes January 2012 from the unknown-break search. Third, a scaling anomaly in March 2012 requires imputation close to the announcement. Fourth, the PCA and entropy indices correlate at only 0.611 and identify different peaks in fragility. Fifth, aggregation hides important differences between local and foreign-owned banks and between institutions that entered the reform above or below the new thresholds. Future research should exploit bank-level treatment intensity, extend the pre-reform series, compare alternative imputation procedures, enumerate all admissible placebo dates and archive both data and code. For now, the value of the paper lies in its disciplined comparison of structural change, not in a causal claim that the available evidence cannot sustain.

Declaration of Competing Interests

The authors declare that they not aware of any competing financial interests or personal relationships that may have influenced the work described in this document.

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Ethical considerations

The article followed all ethical standards appropriate for this kind of research.

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